Shocking experiments with rats. More PCT research

This thread continues where Research_PCT.pdf ends. The dialog on reinforcement theory continues in ReinforcementTheory.pdf

Unedited posts from archives of CSG-L (see INTROCSG.NET):

Date: Tue, 6 Jun 1995 02:44:18 -0600
Subject: Illusion of control

[From Bill Powers (950606.0000 MDT)]

Bruce Abbott (950605.1210 EST) --

> Earlier research had shown that, in rats at least, shock that was controllable was less aversive and had milder physiological effects than otherwise equivalent shock that was uncontrollable.

Problem: what was the difference between shocks that were controllable and shocks that were uncontrollable, when no matter what the rats did the shocks occurred with the same intensity and according to an unalterable schedule? It seems to me that you took great pains to assure that the rats did NOT have any control over the shocks in the experimental condition -- if, that is, your precautions actually succeeded, so the rats' actions had no effect on their experiences of the shocks.

> Earlier research had shown that, in rats at least, shock that was controllable was less aversive and had milder physiological effects than otherwise equivalent shock that was uncontrollable.

In those studies, how did "controllable" shock differ from "equivalent uncontrollable" shock? The only sure way to measure the total intensity of shocks would be to use an integrator to measure the total flow of current through the rat over the time the circuit was supposed to be turned on. An uncontrollable shock would be one that was measured to deliver a fixed total charge through the rat regardless of the rat's actions. Is that what these studies measured?

> But what if just HAVING control is itself a controlled perception? Maintaining that perception might have been "worth" a little extra effort. My study was designed to find out.

If the rats' behavior had no effect on the experienced shocks, then the rats did not actually have any control, did they? The only evidence you could possibly adduce to support the idea that a shock was controllable was that the rat's actions had an effect on it that reduced the average experienced shock rate, duration, or intensity. If the actions have no such effect, then the only correct conclusion is that one has no control.

> There is an apparently common-sensical idea that people who are experiencing stress in their lives should be given more control over the sorts of things that cause the stress, and that this control (even if illusory) will diminish the impact of those events. My research (and a little thought) suggests that this idea is not necessarily true.

I know; Langer has promoted these concepts, and Bill Glasser has decided that her ideas are more germane to his "Control Theory" than mine are. But the concept of illusory control is very hard to test if it does not involve actual control -- that is, actual behavior-dependent mitigation of the effects of stressful stimuli. If you create an illusion of control without giving actual control, the illusion will break down unless you arrange for the stimuli to be reduced whenever appropriate behavior occurs. And in that case, what is the difference between "illusory" and "real" control?

In our experiments where we replayed the cursor positions with the handle disconnected, the "illusion of control" was sustained only as long as the handle movements remained apparently correlated with changes in cursor position. As soon as a cursor movement occurred which was visibly different

from the handle movement that supposedly caused it, the illusion was destroyed. Of course the illusion could be indefinitely sustained if an observer in the background was watching the subject's handle movements and using another handle to make the cursor move in synchronism with the subject's handle movements -- but then the control would be real, if somewhat degraded. The observer would simply become part of the control loop.

The only other way to disguise illusory control would be to make the connection between action and result very uncertain, so it would be difficult to judge whether behavior was having an effect. But in that case, even real control would be hard to detect because the best achievable control would be very poor. It would not be surprising if subjects saw little to choose between have very poor and uncertain control and actually having no control under conditions where it would be hard to detect the lack of control even if you were looking for it.

> Having the perception of control will probably reduce the stressfulness of the situation if your experience suggests that this control will generally lead to an objectively better outcome than if you lacked control. For example, you may feel more relaxed when YOU drive the car than when your teenage son, who has had two weeks of driving experience, is behind the wheel.

Poor example, because both you and your teenage son actually have control of the car when driving. What is stressful about riding with an inexperienced driver is seeing all the errors that are not being corrected. And there are differences in driving style even among good drivers: how far before a stop sign do you start to apply brake pressure, when approaching at 50 mph?

You get the perception of control when deviations in the controlled variable are apparently corrected by your actions. While "real" control may be difficult to verify in some circumstances, lack of control is easy to verify: there is no effect of action on the controlled variable.

> However, having the perception of control will probably _increase_ the stressfulness of the situation if your experience suggests that this control will generally lead to an objectively _worse_ outcome. For example, you might feel much more comfortable allowing the pilot to land the 737 you are flying in than taking the wheel yourself.

Why would having the perception of control increase the stressfulness of the situation if you actually had control? The reason that you prefer to let the pilot land the plane is your knowledge that merely sitting in the pilot's seat and holding the control wheel would NOT give you control over the airplane. You don't know how to fly it, particularly how to land it. Even if you started with the illusion of control (and were thus not bothered by the prospect of landing the plane), you would soon find that it is an illusion, because the plane would not do what you want it to do. Being made responsible for landing the airplane, or thinking you can fly it, does not give you control of the airplane.

> In my study the rats were able to compare the objective outcome under controllable- and uncontrollable-shock conditions and determine that they were the same.

What they discovered was that in either situation, the outcome was uncontrollable. I do not think there were any "controllable" shock conditions.

Best to all, Bill P.

Date: Tue, 6 Jun 1995 11:59:19 -0500
Subject: Control and Stress

[From Bruce Abbott (950606.1155 EST)]

>Bill Powers (950606.0000 MDT)] --

>Bruce Abbott (950605.1210 EST)

>> Earlier research had shown that, in rats at least, shock that was controllable was less aversive and had milder physiological effects than otherwise equivalent shock that was uncontrollable.

> Problem: what was the difference between shocks that were controllable and shocks that were uncontrollable, when no matter what the rats did the shocks occurred with the same intensity and according to an unalterable schedule? It seems to me that you took great pains to assure that the rats did NOT have any control over the shocks in the experimental condition -- if, that is, your precautions actually succeeded, so the rats' actions had no effect on their experiences of the shocks.

Let's look at the simplest situation, which involved escapable versus inescapable shock. When shock occurred in the escapable-shock condition, it continued until terminated by the rat by depressing the "escape" lever. In this condition the rats became very efficient: they remained poised over the escape lever and pressed it rapidly enough to produce shock durations averaging from about 100 to 300 milliseconds for different rats. It is very clear that the rats had control over shock duration in this condition, and that they used that control to minimize shock duration. It is equally clear that the reference level was zero, but that inherent system delays prevented the rats from achieving that reference, although they could come close.

In the inescapable-shock condition, the "escape" lever was retracted and shocks simply lasted a fixed duration (e.g., 100 milliseconds). Here the rats had very little if any control over shock duration. I was interested in determining whether simply HAVING direct (and fairly effective although not perfect) control over shock duration was itself a controlled perception, INDEPENDENT of any objective reduction in shock duration that control would permit. From prior research I had reason to believe that it might.

Now, within the escapable-shock condition it is clear that rats had control over shock duration, yet from the broader perspective of the overall procedure there was no control in that the rats could do nothing to change the objective, overall duration of shock they would receive in a session. I did indeed take great pains to assure that the objective shock outcomes would be identical in the two conditions.

From this you conclude that the outcome of the experiment was unsurprising. But you have not seen the research suggesting that a different outcome was possible, and your expectations are based on nothing more than calculations based on the objective situation. How the rat will actually _perceive_ the situation is unknown. The experiment was needed in order to evaluate the conflicting predictions of alternative theories. (Note: Based on objective calculations of shock parameters, rats should be indifferent between signaled and unsignaled shock, too, and we've already seen how THAT prediction failed.)

>> Earlier research had shown that, in rats at least, shock that was controllable was less aversive and had milder physiological effects than otherwise equivalent shock that was uncontrollable.

> In those studies, how did "controllable" shock differ from "equivalent uncontrollable" shock? The only sure way to measure the total intensity of shocks would be to use an integrator to measure the total flow of current through the rat over the time the circuit was supposed to be turned on. An uncontrollable shock would be one that was measured to deliver a fixed total charge through the rat regardless of the rat's actions. Is that what these studies measured?

The most typical procedure allowed rats to terminate shock by means of some response: by pressing a lever or by jumping into the air, to give two examples. Most of the studies used a matched-pairs design: a second rat would also receive the shock, but for a duration determined by the latency of its partner's escape response. Measuring the total intensity of shock is a rather complex problem because it is not just the total current flow but the current density over the surface of the skin in contact with the grid and a bunch of other factors that determine the sensory qualities of the shock. I don't want to get into a detailed discussion of all the work that has been done to

evaluate these factors; the bottom line is that it's unlikely that differences in the physical effects of shock at the receptor level are responsible for the observed lowered physiological disruption of controllable as opposed to uncontrollable shock.

> If the rats' behavior had no effect on the experienced shocks, then the rats did not actually have any control, did they? The only evidence you could possibly adduce to support the idea that a shock was controllable was that the rat's actions had an effect on it that reduced the average experienced shock rate, duration, or intensity. If the actions have no such effect, then the only correct conclusion is that one has no control.

Again, "no effect" is how YOU perceive the situation, not necessarily how the rat perceives it. WITHIN the two conditions of the experiment, shock WAS controllable in one of them. It is only by comparing the long term, overall shock outcomes in the two conditions that you can conclude that there was no overall ability to control shock duration. Could the rats perceive this? The only way to know for sure was to run the study.

>> Having the perception of control will probably reduce the stressfulness of the situation if your experience suggests that this control will generally lead to an objectively better outcome than if you lacked control. For example, you may feel more relaxed when YOU drive the car than when your teenage son, who has had two weeks of driving experience, is behind the wheel.

> Poor example, because both you and your teenage son actually have control of the car when driving.

News to me. When I'm driving, it seems to me that _I'm_ the one whose foot is on the accelerator and whose hands are on the wheel, not my son. When he's driving, the reverse is true. I can see the uncorrected errors, but unless I'm willing to wrest the wheel from him, I'm operating in open-loop mode--which can be very stressful if this results in large uncorrected errors in my system.

> What is stressful about riding with an inexperienced driver is seeing all the errors that are not being corrected. And there are differences in driving style even among good drivers: how far before a stop sign do you start to apply brake pressure, when approaching at 50 mph?

Precisely the point I was driving at. It's not having or lacking control _per se_ that determines the experienced level of stress, its whether having or lacking control results in the least experienced error. Didn't I make that clear?

>> In my study the rats were able to compare the objective outcome under controllable- and uncontrollable-shock conditions and determine that they were the same.

> What they discovered was that in either situation, the outcome was uncontrollable. I do not think there were any "controllable" shock conditions.

Sometimes Mary drives the car, sometimes you do. In either situation, the perceptual outcome is uncontrollable? Or the outcome is the same: you get to your destination unharmed?

>Rick Marken (950605.2145) --

>> There is an apparently common-sensical idea that people who are experiencing stress in their lives should be given more control over the sorts of things that cause the stress, and that this control (even if illusory) will diminish the impact of those events.

> According to PCT, stress is caused by error -- not things or perceptions of those things.

Hey, cool, STEREO! (;->

> So any lack of control IS a cause of stress (that's why Ed Ford's book about PCT is called, quite appropriately, "Freedom From Stress"; we are free from stress when we are in control).

Wrong. I don't feel terribly stressed while sitting in the passenger seat, so long as I perceive no serious error. Simply lacking control is not stressful. There are a lot of things I can't control and don't worry about. Furthermore, HAVING control can be QUITE stressful if you're not good at correcting error. Try the compensatory tracking task using a high-frequency disturbance and see how unrelaxed it makes you feel.

> You can't really "give" people control, though you might be able to show people how they can improve their ability to control (again, that's what Ed's book is about; ways to improve your own ability to control your perceptions).

I think this raises a definitional problem. What do you mean by "give"? Certainly I can give you the means by which some perception can be controlled, and I can describe how to use those means to achieve control. I can even make it in your best interest to assert control (e.g., I could let go of the steering wheel while we are traveling down the road at 70 mph). What I can't do make you do it.

> If control is illusory (as it was in my closed-open loop experiment) then it is not control -- so it won't help reduce stress (error).

Illusory control may reduce stress (so long as the illusion persists) if what you are worried about is the POSSIBILITY of uncontrollable error. Dumbo's feather is a nice example: he was unwilling to try to fly so long as he believed that the attempt would result in a painful impact with the ground. The feather assured him that he could prevent such an error from arising--by holding onto the feather. Having the illusion that the serious error about which he was worried could not now occur, Dumbo no longer experienced stress about the possible consequences of an attempt at flight.

>> My research (and a little thought) suggests that this idea is not necessarily true.

> I'm not sure that your research really addressed the causes of stress. My interpretation of your results is that the rats were in stress in BOTH conditions: control of shock and no control of shock.

Yeeeessss . . .

> I think that any level of shock greater than 0 (the rat's probable reference level for shock) is stressful for a rat -- it creates error.

Uh-huh . . .

> Thus, your methodology ensured that the rats experienced the same amount of stress in the control of shock and no control of shock conditions.

On the assumption that the only cause of stress is the _objective_ error. The study was designed to provide evidence for or against this assumption. When it comes to a choice between assumptions and data, I prefer data, especially when different researchers are making opposing assumptions.

> I take the rats' failure to reliably select either control or no- control of shock as evidence of reorganization. If the control of shock condition allowed the rat to actually control the shock, keeping it at the reference level (0) then I suspect that the rat would have selected the matching no control of shock condition every time; that way the rat gets the reference level of shock (0) with no work.

No doubt the rats would prefer no shock to the levels they actually received, and would have adopted other, more effective forms of control if they could discover them (as did those exceptional few rats that learned to roll over onto their backs). But I have a real problem with the notion that the rat's

control systems are continually undergoing massive reorganization here. Their behavior appeared to be well organized and adaptive at all times during the study, not continuously varying in some random fashion.

> The observed "indifference" refers (I think) to the fact that the rat selects the control of shock condition 1/2 the time and the no control condition the other 1/2 . This suggests that the rat is reorganizing -- randomly selecting behavioral strategies because the existing strategies are not working; the rat is getting shocked -- it is in stress; it is reorganizing because its current control organization is not working (no thanks to the experimenter, I might add;-)

You didn't read my description of the study very carefully. The rats didn't "select the control of shock condition 1/2 of the time," (although they certainly could have!). What they DID do was stay in whatever condition they were placed into. When the condition was switched, the rats made no attempt to switch back. Nice try, but no cigar.

Regards, Bruce

Date: Tue, 6 Jun 1995 13:38:58 -0700
Subject: Control & Stress

[From Rick Marken (950606.1330)]

Bruce Abbott (950606.1155 EST) --

> HAVING control can be QUITE stressful if you're not good at correcting error.

This statement implies that it is sometimes less stressful to have no control. The only time I can imagine that this would be the case is when you are talking about a variable that doesn't matter to you -- one for which you have no reference specification, such as the temperature of the earth's magma. It's true that poor control of a variable you care about (have a reference for) is stressful; but NO control of that variable is always as bad or worse

> Illusory control may reduce stress (so long as the illusion persists) if what you are worried about is the POSSIBILITY of uncontrollable error. Dumbo's feather is a nice example: he was unwilling to try to fly so long as he believed that the attempt would result in a painful impact with the ground.

Ah. I see what you mean by illusory control. I would call this regular control (no illusions involved); it's just that an outside observer can see that some aspect of the control process is irrelevant. Controlling for holding the feather is part of the means Dumbo uses to control for flight; he has no was of knowing it's irrelevant; only the Disney animators know it's irrelevant. If you try to take the feather away you are disturbing Dumbo's feather control process; this creates error and, hence, stress.

Some stress might also occur because Dumbo IMAGINE'S the result of flying without the feather (which, from his point of view, is the same as flying without ears); the imagined perception is of him falling which is different that what he wants to perceive (staying up). So Dumbo experiences error (stress) as a result of imagining the perception he would get if he flew without a feather; again, stress is error.

Stress (error) that results from the imagined consequences of actions is, indeed, very common (I know from personal experience). The stress is real even if the imagined consequences are not. But I think it's a STRETCH to imagine that rats, deprived of the "illusion" that they are controlling shock, experience stress due to the imagined consequences of this loss.

> I have a real problem with the notion that the rat's control systems are continually undergoing massive reorganization here. Their behavior appeared to be well organized and adaptive at all times during the study, not continuously varying in some random fashion.

I didn't think the reorganization would be "massive" but I did imagine there would be some. But apparently there is little or none because you say:

> What they DID do was stay in whatever condition they were placed into. When the condition was switched, the rats made no attempt to switch back.

I assume that the rats pressed the bar to switch conditions in the early parts of training but eventually stopped. This would be how you know that the rats knew that they could influence the condition they were in, right?

> Nice try, but no cigar.

I'm sorry. I find it hard to concentrate on this kind of research. I'm rooting for the rat.

Best Rick

Date: Tue, 6 Jun 1995 17:18:03 -0600
Subject: Illusion of control

[From Bill Powers (950606.1440 MDT)]

Bruce Abbott (950606.1155 EST) --

> When shock occurred in the escapable-shock condition, it continued until terminated by the rat by depressing the "escape" lever. In this condition the rats became very efficient: they remained poised over the escape lever and pressed it rapidly enough to produce shock durations averaging from about 100 to 300 milliseconds for different rats. It is very clear that the rats had control over shock duration in this condition, and that they used that control to minimize shock duration.

The problem that keeps me from relaxing and enjoying it is that this doesn't seem like a very significant degree of control over the shock. A lot of unanswered questions remain. Is a shock that lasts 100-300 msec essentially zero shock from the rat's point of view, or is it so excruciating that the rat's error signal is approaching saturation for any duration longer than 100 msec? I would much rather have seen an avoidance schedule where the beginning of a trial is signaled and the rat has the ability to prevent the next shock altogether. In that situation, we know that the rat experiences only a few percent of the shock rate that it would experience if it did not press the bar in time. That shows that control definitely exists and is effective.

Then we could play back a recording of the actual shocks, while the rat goes through the same experiment but without the bar presses having any effect on the apparatus. So we have the same behavior and the same shock rate, the only difference being that there is no control at all. I guess what bothers me about your experiment is that whatever control may have existed was pretty weak, and couldn't reduce the shock below some rather long duration. I should think that the rat's error signal was still pretty large even when control was supposedly occurring.

> From this you conclude that the outcome of the experiment was unsurprising. But you have not seen the research suggesting that a different outcome was possible, and your expectations are based on nothing more than calculations based on the objective situation.

I don't know if the output was unsurprising; all I know is that I'm not satisfied with the distinction between control and no control. If the rats were hovering over the bar in the "control" condition, they must still have been experiencing a pretty large error. When I test for control, I want it to be successful. If big errors still remain, we're not seeing very successful control. So the rats must be trying to distinguish between hardly any control and none at all, which, it seems to me, makes telling the difference unnecessarily hard.

> Again, "no effect" is how YOU perceive the situation, not necessarily how the rat perceives it. WITHIN the two conditions of the experiment, shock WAS controllable in one of them. It is only by comparing the long term, overall shock outcomes in the two conditions that you can conclude that there was no overall ability to control shock duration. Could the rats perceive this? The only way to know for sure was to run the study.

Same question: why not make the distinction easy to perceive by letting the rats reduce the incidence of shock close to zero in the controlled case?

RE: illusion of control

>> Poor example, because both you and your teenage son actually have control of the car when driving.

> News to me. When I'm driving, it seems to me that _I'm_ the one whose foot is on the accelerator and whose hands are on the wheel, not my son.

I meant, whichever one of you is driving, that person actually does have control of the car, while the other doesn't.

> It's not having or lacking control _per se_ that determines the experienced level of stress, its whether having or lacking control results in the least experienced error. Didn't I make that clear?

Not really, because "having control" is ambiguous the way you're using the phrase. For example, "Furthermore, HAVING control can be QUITE stressful if you're not good at correcting error."

You seem to use "having control" as meaning "being put in a position where you could control if you knew how", rather than the way I use it, meaning "being able to keep perceptions acceptably close to their desired states by means of actions." The means of control is necessary for having control, but so is the skill, the ability to take advantage of the available means.

> Try the compensatory tracking task using a high-frequency disturbance and see how unrelaxed it makes you feel.

This is an example of lack of control, not of control. You don't have the ability to keep error acceptably small in that situation, so you don't have control even though you're operating the handle. We're just using "having control" in different ways. If a flying instructor turns to the student on his first trip up and says "Ok, you have control now." he's lying. The student may have his hand on the control stick, but he doesn't have control.

You really said this yourself:

> I think this raises a definitional problem. What do you mean by "give"? Certainly I can give you the means by which some perception can be controlled, and I can describe how to use those means to achieve control. I can even make it in your best interest to assert control (e.g., I could let go of the steering wheel while we are traveling down the road at 70 mph). What I can't do is make you do it.

... and what you can't do is call this a "controllable" situation. The only way to demonstrate that something is controllable is to demonstrate control of it.

> Illusory control may reduce stress (so long as the illusion persists) if what you are worried about is the POSSIBILITY of uncontrollable error.

OK, so you tell someone "You're going to get a rather severe shock when that timer counts down to zero. However, if you press this big red button any time after 10 seconds before zero, the shock will be disabled." There's no question of not being able to control the shock; all that's required is a simple act the person knows how to perform. So the level of stress as the countdown

proceeds may climb somewhat, but as soon as the big red button is pressed there is no reason for further stress.

Until the person discovers that this control was illusory, because the big red button wasn't connected to anything and the shock occurred when the timer ran out. This would instantly invalidate the illusion of control and probably everything else the experimenter said from then on.

But suppose that neither the button nor the timer is connected to anything. Now the person presses the button, and when the timer runs out, no shock occurs. It's the same principle as nuclear deterrence or a bear-scarer or taking a prophylactic dose of aspirin or giving up eating eggs. You do something to prevent an uncontrollable error, and sure enough, there is no uncontrollable error. One reasonable hypothesis is that what you did prevented the unwanted error. Belief that you actually controlled the outcome may reduce stress. However, if you use your brain, you will realize that you haven't proven that you have control, and that realization could lead to a great deal of stress. The critical question is not whether you appear to be in control, but whether you ARE in control.

Saying that the illusion of control reduces stress is therefore an iffy statement, because it depends on the gullibility of your subject.

> Dumbo's feather is a nice example: he was unwilling to try to fly so long as he believed that the attempt would result in a painful impact with the ground.

If Dumbo had been real, I would have said he was smarter than the guy who gave him the feather and the story about being able to fly. If you actually can fly, the feather is a bear-scarer. You can't prove it didn't enable you to fly. If you can't fly, however, the illusion of control will be quickly dispelled. That's how the test for the controlled variable works: you can't prove beyond doubt that a particular variable is under control, but you can very quickly show that it's not under control.

>> Thus, your methodology ensured that the rats experienced the same amount of stress in the control of shock and no control of shock conditions.

> On the assumption that the only cause of stress is the _objective_ error. The study was designed to provide evidence for or against this assumption. When it comes to a choice between assumptions and data, I prefer data, especially when different researchers are making opposing assumptions.

I don't see any problem with that assumption, for rats in that experiment. After all, they had direct evidence of lack of control, in that no matter what happened or what they did they still experienced at least 100-300 msec of shock. Even if they had believed they had control over the shock, the next shock would prove they did not.

RE: Rick's suggestion:

> No doubt the rats would prefer no shock to the levels they actually received, and would have adopted other, more effective forms of control if they could discover them (as did those exceptional few rats that learned to roll over onto their backs). But I have a real problem with the notion that the rat's control systems are continually undergoing massive reorganization here.

Why? If a control system is consistently permitting large amounts of error, reorganization is very likely to start up. Seems reasonable to me; it's called "trial and error" behavior. When one behavior isn't working, pretty soon you start trying something else.

> You didn't read my description of the study very carefully. The rats didn't "select the control of shock condition 1/2 of the time," (although they certainly could have!). What they DID do was stay in whatever condition they were placed into. When the condition was switched, the rats made no attempt to switch back.

I missed something. What was it the rats could do to switch from the controllable situation to the uncontrollable one? Was this the house-light situation, too, where another lever would switch the conditions?

From what you say, I deduce that the rats could take some action that would switch conditions. So you were testing to see if they would choose one condition over the other, and they didn't. This says either that they had no preference for being able to control the shocks, or that the difference in degree of control they could obtain in one condition relative to the other was so slight that they couldn't tell the difference.

> Nice try, but no cigar.

Right.

Best to all, Bill P.

Date: Wed, 7 Jun 1995 12:26:56 -0500
Subject: Oddly Familiar

[From Bruce Abbott (950607.1155 EST)]

>Bill Powers (950606.1440 MDT) --

>>Bruce Abbott (950606.1155 EST)

>> When shock occurred in the escapable-shock condition, it continued until terminated by the rat by depressing the "escape" lever. In this condition the rats became very efficient: they remained poised over the escape lever and pressed it rapidly enough to produce shock durations averaging from about 100 to 300 milliseconds for different rats. It is very clear that the rats had control over shock duration in this condition, and that they used that control to minimize shock duration.

> The problem that keeps me from relaxing and enjoying it is that this doesn't seem like a very significant degree of control over the shock. A lot of unanswered questions remain. Is a shock that lasts 100-300 msec essentially zero shock from the rat's point of view, or is it so excruciating that the rat's error signal is approaching saturation for any duration longer than 100 msec?

In the absence of responding, each shock terminated automatically at 15 seconds. For a rat able to keep the average shock length at 150 milliseconds (0.15 seconds, control reduces the disturbance (shock) to 1% of its uncontrolled value. By your definition this is excellent control, yes?

> I would much rather have seen an avoidance schedule where the beginning of a trial is signaled and the rat has the ability to prevent the next shock altogether. In that situation, we know that the rat experiences only a few percent of the shock rate that it would experience if it did not press the bar in time. That shows that control definitely exists and is effective.

> Then we could play back a recording of the actual shocks, while the rat goes through the same experiment but without the bar presses having any effect on the apparatus. So we have the same behavior and the same shock rate, the only difference being that there is no control at all.

Hmmmm, this sounds oddly familiar. Could it be . . .

>>Bruce Abbott (950603.1340 EST)

>> In the avoidance experiment the rat performed on a Sidman shock-avoidance schedule on one session and the actual temporal pattern of shock delivery was recorded (rats on this schedule occasionally make mistakes and receive shocks); this pattern was "played back" on the next session, in which the rat had no control over shock delivery. As with escapable

> versus inescapable shock schedules, the rats failed to resist the disturbance when the apparatus switched them from avoidable to unavoidable shock schedules or vice versa. The key here is that during training the rats had learned that the shock frequency was the same whether they controlled shock delivery let the apparatus determine when shocks would be delivered.

Will that do?

> I guess what bothers me about your experiment is that whatever control may have existed was pretty weak, and couldn't reduce the shock below some rather long duration. I should think that the rat's error signal was still pretty large even when control was supposedly occurring. . . . If the rats were hovering over the bar in the "control" condition, they must still have been experiencing a pretty large error.

What? If your participants keep their hands on the mouse during the compensatory tracking task, "they must still have been experiencing a pretty large error"? Hmmm.

> When I test for control, I want it to be successful. If big errors still remain, we're not seeing very successful control. So the rats must be trying to distinguish between hardly any control and none at all, which, it seems to me, makes telling the difference unnecessarily hard.

Even in the escapable versus inescapable shock experiment the rats had no difficulty distinguishing when they did and did not have direct control over shock duration. It's pretty obvious: in the escapable shock condition the escape lever is extended and, unless you press it, shock just keeps goin' and goin' and goin' (up to 15 seconds). In the inescapable shock condition the lever is retracted (so there is nothing to press) and the shock stops after a brief, fixed duration, independent of behavior.

> . . .why not make the distinction easy to perceive by letting the rats reduce the incidence of shock close to zero in the controlled case?

Ah, but I did.

>>> Thus, your methodology ensured that the rats experienced the same amount of stress in the control of shock and no control of shock conditions.

>> On the assumption that the only cause of stress is the _objective_ error. The study was designed to provide evidence for or against this assumption. When it comes to a choice between assumptions and data, I prefer data, especially when different researchers are making opposing assumptions.

> I don't see any problem with that assumption, for rats in that experiment. After all, they had direct evidence of lack of control, in that no matter what happened or what they did they still experienced at least 100-300 msec of shock. Even if they had believed they had control over the shock, the next shock would prove they did not.

That depends in whether they were in the escapable or inescapable shock condition at the time. In the former condition, the shock duration was clearly under the rat's control.

No real control system can keep the controlled perception exactly at reference at all times regardless of disturbance. The shock disturbance in the escape condition is a step function and the system response to error (lever depression) cancels the disturbance completely and in very short order. I'd call that effective control, wouldn't you? To maintain that control, the rat must remain poised over the lever, ready to respond, just as the compensatory tracker must keep his or her hand on the mouse.

The crux of the experiment is that the rat can get the same perceptual outcome (in terms of average shock duration) whether IT controls shock duration directly or just lets the apparatus program the shock durations. Objectively (and only over the long run), there is nothing to choose between (other than

whether to work or not work). There are conditions in which a rat will work to earn food pellets even though it must stand on a pile of them to do so, so we can't just assume we know what the rat will prefer in this case. My study demonstrated that rats can indeed compare overall shock exposure in the two conditions and, when those exposures are the same, do not act to control which condition they are in.

> What was it the rats could do to switch from the controllable situation to the uncontrollable one? Was this the house- light situation, too, where another lever would switch the conditions?

Yes. On some test sessions, a press on the "changeover" lever switched the rat from escapable to inescapable shock conditions; on others the reverse was true.

> From what you say, I deduce that the rats could take some action that would switch conditions. So you were testing to see if they would choose one condition over the other, and they didn't. This says either that they had no preference for being able to control the shocks, or that the difference in degree of control they could obtain in one condition relative to the other was so slight that they couldn't tell the difference.

>> Nice try, but no cigar.

> Right.

Right? I think I've shown that the second possibility can be ruled out. The first possibility _was the conclusion I drew from the results_. I'm not disagreeing with your conclusion, you're agreeing with mine. Congratulations, you get the cigar.

>RE: Rick's suggestion:

>> No doubt the rats would prefer no shock to the levels they actually received, and would have adopted other, more effective forms of control if they could discover them (as did those exceptional few rats that learned to roll over onto their backs). But I have a real problem with the notion that the rat's control systems are continually undergoing massive reorganization here.

> Why? If a control system is consistently permitting large amounts of error, reorganization is very likely to start up. Seems reasonable to me; it's called "trial and error" behavior. When one behavior isn't working, pretty soon you start trying something else.

I gave my answer in the last sentence of the quoted paragraph:

>> Their behavior appeared to be well organized and adaptive at all times during the study, not continuously varying in some random fashion.

With some experience in this procedure, the rats settled into a routine set of behaviors exhibiting little variability.

Regards, Bruce

Date: Wed, 7 Jun 1995 14:00:22 -0500
Subject: Having Control

[From Bruce Abbott (950607.1355 EST)]

>Bill Powers (950606.1440 MDT) --
>>Bruce Abbott (950606.1155 EST)

>>> You seem to use "having control" as meaning "being put in a position where you could control if you knew how", rather than the way I use it, meaning "being able to keep perceptions acceptably close to their desired states by means of actions." The means of control is necessary for having

> control, but so is the skill, the ability to take advantage of the available means.

>> Try the compensatory tracking task using a high-frequency disturbance and see how unrelaxed it makes you feel.

> This is an example of lack of control, not of control. You don't have the ability to keep error acceptably small in that situation, so you don't have control even though you're operating the handle. We're just using "having control" in different ways. If a flying instructor turns to the student on his first trip up and says "Ok, you have control now." he's lying. The student may have his hand on the control stick, but he doesn't have control.

I agree that we are using this phrase differently. What we need is a better vocabulary! But I disagree with your definition, or at least in your application of it to this example, wherein you conclude that control was lacking. Your definition of "having control" suggests that one either has or does not have control, even though, as we are both well aware, there are all degrees in between. My performance may be such that I am reducing the effect of the disturbance on the controlled perception, but not really doing a good job of it (overshoots, undershoots, etc.) Do I have control or not? A given performance lies along a continuum of control, from total out-of-control to extremely well controlled.

My example was intended to convey the point that having control can be stressful if maintaining control is difficult. You may be doing a good job, but you are working at the edge of your abilities, or you may be working under conditions in which control is beginning to suffer (e.g., keeping up with the lower-frequency components but not the high-frequency ones).

As you noted, the way I used the phrase "having control" was to convey having the MEANS of control (access to the required variables), which is only a prerequisite to "having control" as you define it. For clarity it would be nice to have a separate descriptor for this prerequisite condition. Both meanings of "have control" seem to be in common use. Would "have the means to control" provide the needed distinction?

Also, this difference in meaning suggests that distinctions can be drawn among three situations in which a perception is not in control: (a) you do not have the means of control and therefore are not attempting to control it, (b) you have the means of control but are not attempting to control, and (c) you have the means of control and are attempting to establish control, but have not yet succeeded. In cases (a) and (b) the variable is "not in control," in case (c) it is "out of control." In cases (a) and (b) the individual is doing nothing, in case (c) he or she is engaged in a frantic effort to achieve control.

Regards, Bruce

Date: Wed, 7 Jun 1995 15:45:12 -0700
Subject: Controlled Perceptions

[From Rick Marken (950607.1545)]

Bruce Abbott (950607.1355 EST)--

> My example was intended to convey the point that having control can be stressful if maintaining control is difficult.

To the extent that control is not maintained there is error -- and stress IS error. But NOT having control is always AT LEAST as bad as having it, and usually FAR worse.

Bruce Abbott (950607.1155 EST) --

> No real control system can keep the controlled perception exactly at reference at all times regardless of disturbance.

The phrase "regardless of disturbance" is ambiguous. Do you mean "given disturbances that change over time" or "no matter what the disturbance happens to be"? If you mean the latter, then the statement is obviously not true since a real control system can keep p exactly equal to r as long as d is a constant.

Bruce Abbott (950607.1245 EST) --

> Hmmmm. Rick, you don't have a CLUE as to what my motives are.

You are correct. I don't know your motives because there is no such thing as a "motive". "Motive" is a dormitive principle that was used to explain behavior before people understood the nature of behavior as the control of perception. People don't have motives any more than flammable materials have phlogiston.

What you do have are perceptions that you are controlling. I think I have a pretty good idea what some of those perceptions are. It's impossible to hide the perceptions one is controlling from someone who knows how to find out what they are (using the Test). That's what I meant when I said (950606.1900):

> When you know what to look for, controlled perceptions are really quite obvious. Of course, Bill Powers (950606.1440 MDT) picked up on it too.

I'm not saying that I know EXACTLY what perceptions you are controlling; and even if I did know your controlled perceptions exactly you might reject my description of them because you don't like the words I use to describe them (you could be controlling for using particular words to describe your goals); or you might not like acknowledging, for whatever higher level reason, what you are controlling (you might be controlling for "privacy of purpose").

But if you're interested, I'd be happy to tell you what perceptions I think you are controlling. I would imagine that you could do a pretty good job of telling me what perceptions I am controlling, too. You did a nice job of describing some of those perceptions when you answered my question about why you thought I was against spending much time trying to explain the findings of conventional psychology using PCT.

Best Rick

Date: Wed, 7 Jun 1995 22:25:02 -0400
Subject: Re: Oddly Familiar

<[Bill Leach 950607.21:23]
>[From Bruce Abbott (950607.1155 EST)]

Bruce I would suggest that it may be necessary to run the experiment over again but with a ramped shock onset condition or the already suggested (by Bill P.) idea to provide a warning signal sufficiently prior the to the onset of the shock in order to effectively answer the challenges to your conclusions.

I admit that this is involving some guesses on my part but it seems that if a ramp rate could be selected such that the onset of the shock is perceived prior to the shock becoming "annoying" such that the rat could "choose" to turn it off or not.

The assumption that a 1% duration shock is linearly equivalent to a 100% duration shock in terms of disturbance is stretching credibility in my opinion.

From personal experience with shocks (unfortunately for me... considerable), the most memorable component was that I was shocked at all. There are a couple that I know must have been of _VERY_ short duration based upon the physical fact that I am still with us (first was a solid connection to a relatively "hard" 300 VDC and circuit common and the second was a very solid connection to two phases of a 450 VAC distribution bus. My own anecdotal evidence is that as far as perception is concerned, the intensity of the shock is a far greater concern than the duration. Indeed, if the intensity is low

enough, the shock experience can actually have pleasant perceptions associated with it.

I do not have any "hard" data for this assertion with respect to shock but there are several observed characteristics of other perceptions which, in my mind, support the possibility of the idea.

The first is that perceived luminous intensity for a flashing light source (flashing much faster than the high level perceptual system can handle) is that the perceived intensity is a function of peak intensity and is almost completely independent of duty cycle. Of course none of the mentioned parameters in this case are linear. I believe that perceived intensity is a log function similar to sound intensity.

The second is that most testing involving sensory input at a high level (ie: approaching maximum dynamic input range) very quickly appear to loose a major amount of their input gain. Thus, the perception of a 15 second steady state shock current might be that of a differentiator (with some lossey DC bypass).

I am not claiming that these potential problems are any more than just that... potential problems.

-bill

Date: Wed, 7 Jun 1995 23:14:58 -0600
Subject: Bruce's experiments

[From Bill Powers (950607.2010 MDT)]

Bruce Abbott (950607.1155 EST) --

> In the absence of responding, each shock terminated automatically at 15 seconds. For a rat able to keep the average shock length at 150 milliseconds (0.15 seconds, control reduces the disturbance (shock) to 1% of its uncontrolled value. By your definition this is excellent control, yes?

Depends. A formal observational definition of a reference level for a controlled variable is that level of the variable at which the system's action just drops to zero. To find the reference level for the shocks, you'd have to do something to reduce them until the rats wouldn't bother to act to avoid them. Then you could judge the loop gain by seeing how much you would have to raise the shocks again to get the maximum amount (speed, whatever) of action against them. That would define the range of control, and you could then guess how important a given level of shock is to the rat by measuring the ratio of error to effort. I suppose this would entail adjusting the shock current, since the rats have a minimum reaction time and you don't know if they've reached it.

> Hmmmm, this sounds oddly familiar. Could it be . . .

> In the avoidance experiment the rat performed on a Sidman shock-avoidance schedule on one session and the actual temporal pattern of shock delivery was recorded (rats on this schedule occasionally make mistakes and receive shocks); this pattern was "played back" on the next session, in which the rat had no control over shock delivery. As with escapable versus inescapable shock schedules, the rats failed to resist the disturbance when the apparatus switched them from avoidable to unavoidable shock schedules or vice versa. The key here is that during training the rats had learned that the shock frequency was the same whether they controlled shock delivery let the apparatus determine when shocks would be delivered.

> Will that do?

Depends. Did the rats demonstrate that they knew how to switch from one schedule to the other? For example, if you doubled the recorded shock rate, would they then switch to the controlled condition, and if you halved the

recorded shock rate would they switch to the uncontrolled condition? I'm presuming that the rats would show a preference for the lower shock rate, even if they weren't controlling for controlling. If they didn't switch to the lower shock rate, maybe they never understood the switching concept.

Also, I presume that whenever the "controlled" condition appeared, the rats reduced the experienced shock rate by doing whatever they had to do. Perhaps their failure to differentiate indicates that whatever the shock rate was in the controlled condition, it was below the level they considered an unacceptable amount of error. In the experiments I analyzed, the rats were holding the shock rate as low as a few shocks per hour. We could explain the failure to switch conditions as indicating that the shock rate achieved in the controlled condition was experienced as zero error. The same shock rate, recorded, would also be experienced as zero error, or small enough not to bother with.

>> If the rats were hovering over the bar in the "control" condition, they must still have been experiencing a pretty large error.

> What? If your participants keep their hands on the mouse during the compensatory tracking task, "they must still have been experiencing a pretty large error"? Hmmm.

Depends. If they're leaning toward the screen and moving the mouse as fast as they can, I assume they still think the errors they're seeing are pretty large. When you reduce the amplitude of the disturbance, so the cursor only moves a fraction of an inch from the target at most, subjects relax and don't try so hard. That's just an impression, of course. We can judge how much error they're perceiving by seeing what amount of error will just lead them to stop moving the mouse.

>> When I test for control, I want it to be successful. If big errors still remain, we're not seeing very successful control. So the rats must be trying to distinguish between hardly any control and none at all, which, it seems to me, makes telling the difference unnecessarily hard.

> Even in the escapable versus inescapable shock experiment the rats had no difficulty distinguishing when they did and did not have direct control over shock duration. It's pretty obvious: in the escapable shock condition the escape lever is extended and, unless you press it, shock just keeps goin' and goin' and goin' (up to 15 seconds). In the inescapable shock condition the lever is retracted (so there is nothing to press) and the shock stops after a brief, fixed duration, independent of behavior. z

Depends. It's pretty obvious to you, but it is obvious to a rat? Suppose that to a rat, a 100 millisecond shock (of the intensity used) creates 95% of the maximum possible sensation of being shocked, and any shock longer than 300 milliseconds raises the perception to the maximum value the perceptual signal can have -- 100% of the maximum sensation. That's a 5% different in perception. So whether the shock lasts 300 msec or 15 seconds makes no difference in the perceptual signal (or the error, or the output): it's saturated.

I'm not saying that's true. But without testing, how can you rule it out? The only real way to find out if the rat is controlling successfully is to find the level of the controlled variable at which the rat would just cease trying to correct it, and then the level at which it couldn't try any harder.

> That depends on whether they were in the escapable or inescapable shock condition at the time. In the former condition, the shock duration was clearly under the rat's control.

Depends. The rats were clearly _trying_ to control the shock when they could. They succeeded in getting its duration down to 100-300 milliseconds. While I don't know anything about a rat's reaction time to a shock, that strikes me as getting close to the fastest possible reaction time. This could be tested, of course: you could double the shock intensity and see if they reduced the duration to 50-150 milliseconds, and if you halved it they increased it to

200-600 milliseconds (not literally proportionally, of course). The point is to find out whether they're trying as hard as they can and still experiencing a very large error, or whether they're successfully reducing the shock duration to just the duration they prefer. To experience maximum control means experiencing a match between the perception and its reference level.

> No real control system can keep the controlled perception exactly at reference at all times regardless of disturbance.

True. The problem is how to decide what is a "large" error and what is a "small" one. The best way to judge is in terms of the whole control range. The control range can be determined by seeing how much error is needed to produce the maximum possible effort in opposition to it.

If you see that the effort is at its maximum, then you know that you are on the edge of the controller's range of control -- or perhaps beyond it. If you're beyond it, increasing the disturbance further will not increase the effort further; it can't get any larger. When that is observed, you know that the system is trying to control, but is not actually able to control. The only way to verify that the system is really controlling is to vary the disturbance and see if the effort varies in opposition to it. If you find that an increase in shock intensity results in a substantially shorter reaction time by the rats, then you know that they were controlling, rather than just trying to control.

An analogy. Suppose you were told that by pressing a lever with one hand, you could cause the immediate withdrawal of a plunger on which is mounted a needle that is inserted under one fingernail on your other hand (which is immobilized). At the beginning of each trial, the plunger moves forward in 5 milliseconds, driving the needle 1/2 centimeter into the interface between fingernail and finger tissues. If you could react to press the button in 250 milliseconds, would you consider that you are in control of this experience? If you could keep your free hand just over the button and by intense concentration reduce your reaction time to 150 milliseconds, would you now feel in control?

I wonder if the rats ever try hammering on the lever as fast as they can, and if the experimenter has to think of a way to keep them from doing that so he can measure the true reaction time. I think you might come up with that strategy as a subject in the above experiment. I wonder if the rats ever vocalize when they get shocked, and what a recording of that vocalization might sound like if slowed down so the mean pitch was, say, 500 hz. I wonder if you might vocalize in the above experiment.

> My study demonstrated that rats can indeed compare overall shock exposure in the two conditions and, when those exposures are the same, do not act to control which condition they are in.

Try that out with my hypothetical experiment above. The needle could be withdrawn by you in one condition, and in the other condition would automatically withdraw after 200 millisec. Would you be thinking in terms of whether you have control or not? I rather think that any cognitive functions you might have would be pretty well shut down. This tends to happen under torture.

>> This says either that they had no preference for being able to control the shocks, or that the difference in degree of control they could obtain in one condition relative to the other was so slight that they couldn't tell the difference.

> Nice try, but no cigar.

Right.

> Right? I think I've shown that the second possibility can be ruled out.

I don't think you have ruled it out, unless there is some other observation you made that you haven't told me about. We don't know that the rats could have shortened their reaction times if they had wanted to reduce the duration

even more. Which is better, having no control at all, or trying as hard to control as you can and failing?

> My performance may be such that I am reducing the effect of the disturbance on the controlled perception, but not really doing a good job of it (overshoots, undershoots, etc.) Do I have control or not? A given performance lies along a continuum of control, from total out-of-control to extremely well controlled.

I agree. The best sense of control comes when you're keeping the controlled variable exactly where you want it. Loss of control is sensed when you're trying as hard as you can and the errors are still unacceptably large. As you say, there is a continuum of "being in control;" it's not an either-or thing. That's what I was talking about above: trying to determine where the rats really were on this continuum. I don't blame anyone for not making this determination when the experiments were done, but the determination was not made.

> Would "have the means to control" provide the needed distinction?

It would certainly help.

> Also, this difference in meaning suggests that distinctions can be drawn among three situations in which a perception is not in control: (a) you do not have the means of control and therefore are not attempting to control it, (b) you have the means of control but are not attempting to control, and (c) you have the means of control and are attempting to establish control, but have not yet succeeded. In cases (a) and (b) the variable is "not in control," in case (c) it is "out of control."

Good distinctions.

> In cases (a) and (b) the individual is doing nothing, in case (c) he or she is engaged in a frantic effort to achieve control.

Right. We can refine this even a little further, in that what seems like "frantic" action to us may still be within the control range, although nearing the state of zero control. The step beyond (c) would be "futile" efforts, meaning efforts that are too small to oppose all of the disturbance, yet that are the maximum efforts that can be produced.

Going back (hard to keep up with all this):

Bruce Abbott (950606.1800) [on the related subject "Cherry Pie]

> For the "blind tracking" test you have them wear a blindfold, but you discover that two of your subjects are cheating by peeking under the blindfold. What should you do? You should:

> (a) allow them to cheat because this is their preferred mode of control over cursor position.

> (b) prevent them from cheating, perhaps by taking steps to assure that they will not be able to peek.

> (c) abandon the research because you have determined that the question is silly, as the nonvisual mode of control is not the one the participants would select for themselves. As for me, I'd shave their backs. (;->

I would ask them to reconsider their agreement to be part of this experiment, explain what I am trying to find out, and ask if they are willing to play it straight. If they are, we can go ahead; if not, I can get other subjects. I would not shave their backs.

Actually, what I DID was to turn off the display, so the question of peeking never arose.

Those rats who flipped over on their backs would be worth studying. Were they smarter than the other rats, or just lucky enough to have fallen over at the right time and alert enough to realize the effect on the shocks? If I found myself starting to play "behave, dammit" with a bunch of rats, I think I'd have to pause for a little self-examination, and maybe even have a critical look at previous data. And if I overlooked an interesting phenomenon because I wanted to get on with my idea of what was important, I'd have to turn in my serendipity license.

Being lazy, I would like to do experiments with rats in which they would wander over to the apparatus now and then and work it to get something they want, then go on about their business. Of course this would mean that whatever they were doing must have seemed to the rats worth doing. Associating shocks with the apparatus wouldn't help achieve this.

Best, Bill P.

Date: Thu, 8 Jun 1995 11:07:17 -0500
Subject: Shocking Details

[From Bruce Abbott (950608.1105 EST]

>Bill Leach 950607.21:23 --
>>[From Bruce Abbott (950607.1155 EST)]

> Bruce I would suggest that it may be necessary to run the experiment over again but with a ramped shock onset condition or the already suggested (by Bill P.) idea to provide a warning signal sufficiently prior the to the onset of the shock in order to effectively answer the challenges to your conclusions.

> I admit that this is involving some guesses on my part but it seems that if a ramp rate could be selected such that the onset of the shock is perceived prior to the shock becoming "annoying" such that the rat could "choose" to turn it off or not.

Bill's suggestion was intended to permit the rat to avoid the shocks rather than to escape more efficiently from them. The experiment suggested by Bill was in fact done in that study. The rats were trained on a Sidman avoidance schedule. In the absence of responding on the avoidance lever, they received a 0.5 second duration footshock every 5 seconds. However, each lever-press reset the shock timer so that the next shock was always 20 seconds from the last press. By pressing the lever at least once within this 20-second interval, the rats theoretically could avoid every shock. If a further response did not occur within the 20 seconds, a shock was delivered, and further shocks were then delivered at 5-second intervals until another response occurred. The rats became very efficient controllers of shock frequency on this schedule, receiving only a few shocks in an hour's time. However, when given a choice between the avoidable-shock condition and one in which shocks were unavoidable (but delivered in the same number and pattern as the avoidable shocks, the rats showed no preference.

Now, a finding of no-preference could indicate either that the rats were indifferent between the conditions or that they had not learned how to switch from one condition to the other. To rule out the latter possibility, I added a warning signal just prior to shock to shocks in _one_ of the two conditions (avoidable or unavoidable). I knew from previous research that rats prefer signaled over unsignaled shock; if the rats knew how to switch conditions they would now be expected to do so if switching put them into the condition offering the signaled shocks. They did. The rats chose signaled avoidable shock over unsignaled unavoidable shock, and they chose signaled unavoidable shock over unsignaled avoidable shock. They controlled for signaled shock, but not for avoidable shock.

> The assumption that a 1% duration shock is linearly equivalent to a 100% duration shock in terms of disturbance is stretching credibility in my opinion.

I didn't make that assumption. What I said was that the rats in the escapable condition (of the escapable versus inescapable shock experiment) were able to cut the duration of shock from 15 seconds to around 0.15 seconds by means of their behavior, or to 1% of its uncontrolled value.

> From personal experience with shocks (unfortunately for me... considerable), the most memorable component was that I was shocked at all. There are a couple that I know must have been of _VERY_ short duration based upon the physical fact that I am still with us (first was a solid connection to a relatively "hard" 300 VDC and circuit common and the second was a very solid connection to two phases of a 450 VAC distribution bus. My own anecdotal evidence is that as far as perception is concerned, the intensity of the shock is a far greater concern than the duration. Indeed, if the intensity is low enough, the shock experience can actually have pleasant perceptions associated with it.

This is a serious misconception many people have about the nature of the shocks used in experiments such as mine. Most of us have had the misfortune of experiencing contact with line voltage (or worse), definitely a memorable experience. We've heard of electric shock being used as a means of torture (usually the culprits are depicted as Nazis or, of course, Mad Scientists). It's an easy step to thinking that what experimental psychologists use when they deliver shock to their hapless subjects is something akin to torture. Not so.

The shock you experienced when you came into contact with both sides of a 300 VDC source most likely involved a fairly high current flow (limited only by the current-sourcing capacity of the supply and your own bodily resistance). Skin resistance drops as current flow through the surface of the skin increases, a positive feedback relationship leading to a rapidly increasing current flow over time. If there's enough current for a long enough time it can kill you; more typically it can jolt your muscles into extreme contraction. If you contact a power line with your palm, this can cause your fingers to close around the wire and you won't be able to let go. This is nothing like what is used in psychology experiments.

What is used in the laboratory as a source of "aversive stimulation" is typically a current-regulated source set to around 1.0 milliamps (one thousandth of an ampere). This level is sufficiently strong that a rat will acquire a response that prevents or terminates the shock. It is clearly aversive to the rat but far from traumatizing. Rats maintain their appetites during the study, are not unusually susceptible to disease, and behave when handled in the same ways rats with no shock experience behave.

The 1.0 mA level is close to the minimum value that will produce rapid, reliable escape behavior. At 0.5 mA the escape latencies are longer as the rats are likely to be spending more of their time exploring the chamber, grooming, and so on, and thus tend to be some distance from the escape lever when the shock occurs. At 0.25 mA they may not acquire the escape response at all.

I have regularly administered 1.0 Ma shocks to myself in order to confirm that the shock grid is working properly. The first time you try this you tend to overreact (a phenomenon called fear-potentiated startle) but you quickly discover that it's not all that bad, although you clearly don't like it and would rather not have the experience.

The idea some people have about the shocks administered in these studies has a parallel in their idea of what goes on in appetitive (food) studies. The picture conjured up here is that of concentration-camp victims, starved down to skin and bone. A picture that is closer to the truth is that of someone who hasn't eaten since noon yesterday: hungry, certainly willing to work for some supper, but not in agonizing even-eat-the-shoeleather hunger of 18th century sailors stuck in the doldrums of the Sargasso Sea. The purpose of both the food deprivation and shock levels is simply to provide the motivation

for the behaviors the investigator wishes to study. To use higher levels would defeat the purpose of the study. A rat cringing in terror in the corner of an operant chamber is hardly going to learn to press a lever that prevents shocks from occurring, and one enduring semi-starvation will probably be too busy searching for food to pay attention and learn fine distinctions on a discrimination task.

Occasionally we do hear about experiments that used extreme conditions, but these are the rare exceptions. Such studies are only undertaken after considerable soul-searching to decide whether such treatment is ethically justifiable, and only with the approval of a properly constituted, federally mandated review board.

> The first is that perceived luminous intensity for a flashing light source (flashing much faster than the high level perceptual system can handle) is that the perceived intensity is a function of peak intensity and is almost completely independent of duty cycle. Of course none of the mentioned parameters in this case are linear. I believe that perceived intensity is a log function similar to sound intensity.

Perceived shock intensity is roughly a log function of physical intensity, although there are a host of other factors involved (which have been investigated; I won't get into them here).

> The second is that most testing involving sensory input at a high level (ie: approaching maximum dynamic input range) very quickly appear to loose a major amount of their input gain. Thus, the perception of a 15 second steady state shock current might be that of a differentiator (with some lossey DC bypass).

The psychophysics of both shock intensity and duration have been investigated. The intensity function is steeper than the duration function. As rats can clearly differentiate shocks of different durations, the input function is not that of a differentiator. It's a log function like that of intensity.

Regards, Bruce

Date: Thu, 8 Jun 1995 13:02:13 -0500
Subject: Catch-22

[From Bruce Abbott (950608.1300 EST)]

> Bill Powers (950607.2010 MDT) --
>> Bruce Abbott (950607.1155 EST)

>> In the absence of responding, each shock terminated automatically at 15 seconds. For a rat able to keep the average shock length at 150 milliseconds (0.15 seconds, control reduces the disturbance (shock) to 1% of its uncontrolled value. By your definition this is excellent control, yes?

> Depends. A formal observational definition of a reference level for a controlled variable is that level of the variable at which the system's action just drops to zero. To find the reference level for the shocks, you'd have to do something to reduce them until the rats wouldn't bother to act to avoid them. Then you could judge the loop gain by seeing how much you would have to raise the shocks again to get the maximum amount (speed, whatever) of action against them. That would define the range of control, and you could then guess how important a given level of shock is to the rat by measuring the ratio of error to effort. I suppose this would entail adjusting the shock current, since the rats have a minimum reaction time and you don't know if they've reached it.

Studies something of the nature you describe have been done, although I would like to see them done better in the way you describe. Shock aversiveness is proportional to something like a weighted product of intensity and duration, with intensity having the greater weight. At the intensity I used in this

study I think its safe to assume that reference for shock duration is lower than they were able to achieve.

From one viewpoint the rat is controlling shock duration, presumably to some reference at or close to zero. This control is very effective in that shock duration is reduced to something on the order of 1% of its uncontrolled duration. You seem to be suggesting that the residual 1% may be so aversive that 1% and 100% are basically equivalent from the rat's point of view. If that is true, why are they controlling at all?

>> In the avoidance experiment the rat performed on a Sidman shock-avoidance schedule on one session and the actual temporal pattern of shock delivery was recorded (rats on this schedule occasionally make mistakes and receive shocks); this pattern was "played back" on the next session, in which the rat had no control over shock delivery. As with escapable versus inescapable shock schedules, the rats failed to resist the disturbance when the apparatus switched them from avoidable to unavoidable shock schedules or vice versa. The key here is that during training the rats had learned that the shock frequency was the same whether they controlled shock delivery let the apparatus determine when shocks would be delivered.

>> Will that do?

> Depends. Did the rats demonstrate that they knew how to switch from one schedule to the other?

Yes (see today's post from me to Bill Leach relative to the avoidance study). In the escapable- versus inescapable-shock study, I parametrically varied the duration of inescapable shock across sessions (the duration was fixed within-session). When the inescapable shocks were about 100 milliseconds or more longer than the _average_ duration of the escapable shocks, the rats preferred the escapable-shock condition. When the reverse was true they preferred the inescapable-shock condition. When the shock durations in the two conditions matched within 100 milliseconds, preference, if any, was below the sensitivity of the experiment to detect. The rats knew what they preferred, and they knew how to get it.

> Also, I presume that whenever the "controlled" condition appeared, the rats reduced the experienced shock rate by doing whatever they had to do. Perhaps their failure to differentiate indicates that whatever the shock rate was in the controlled condition, it was below the level they considered an unacceptable amount of error. In the experiments I analyzed, the rats were holding the shock rate as low as a few shocks per hour. We could explain the failure to switch conditions as indicating that the shock rate achieved in the controlled condition was experienced as zero error. The same shock rate, recorded, would also be experienced as zero error, or small enough not to bother with.

This, of course, opposite to your earlier suggestion that the rats did not prefer control because it left and UNACCEPTABLE level of error. So either way, you propose that the experiment fails. Catch-22.

>>> If the rats were hovering over the bar in the "control" condition, they must still have been experiencing a pretty large error.

>> What? If your participants keep their hands on the mouse during the compensatory tracking task, "they must still have been experiencing a pretty large error"? Hmmm.

> Depends. If they're leaning toward the screen and moving the mouse as fast as they can, I assume they still think the errors they're seeing are pretty large.

Depends. They could be nearsighted and doing an excellent job of correcting some rapidly varying disturbances. [Hey, I can play this game, too! (;->]

You asserted that the rats MUST be experiencing (strong residual) error because they were staying on the lever. My example shows that this assertion

is wrong: they COULD be doing an effective job of controlling. That they might not be is irrelevant in this context.

>> Even in the escapable versus inescapable shock experiment the rats had no difficulty distinguishing when they did and did not have direct control over shock duration. It's pretty obvious: in the escapable shock condition the escape lever is extended and, unless you press it, shock just keeps goin' and goin' and goin' (up to 15 seconds). In the inescapable shock condition the lever is retracted (so there is nothing to press) and the shock stops after a brief, fixed duration, independent of behavior. z

> Depends. It's pretty obvious to you, but it is obvious to a rat? Suppose that to a rat, a 100 millisecond shock (of the intensity used) creates 95% of the maximum possible sensation of being shocked, and any shock longer than 300 milliseconds raises the perception to the maximum value the perceptual signal can have -- 100% of the maximum sensation. That's a 5% different in perception. So whether the shock lasts 300 msec or 15 seconds makes no difference in the perceptual signal (or the error, or the output): it's saturated.

Research is not conducted in a vacuum. There's plenty of independent evidence that rats can discriminate such differences, including the internal evidence from the experiment itself regarding discrimination of shock duration differences. Psychophysical studies show that saturation is not a problem: perception of shock duration follows Weber's Law.

>> That depends on whether they were in the escapable or inescapable shock condition at the time. In the former condition, the shock duration was clearly under the rat's control.

> Depends. The rats were clearly _trying_ to control the shock when they could.

I think we're splitting hairs here. They clearly were doing more than trying. Note what happens to shock intensity when a response occurs: it instantly falls from a rather aversive level to zero.

> The only way to verify that the system is really controlling is to vary the disturbance and see if the effort varies in opposition to it. If you find that an increase in shock intensity results in a substantially shorter reaction time by the rats, then you know that they were controlling, rather than just trying to control.

If you are correct, then there is no way to test for control of a logical variable when the output is a step function.

I don't want you to get the impression that I miss your point. Control over shock termination is not ideal because it cannot prevent the shock from occurring in the first place (especially when shock is programmed as a step disturbance as it typically is in such experiments). I had the same concern you have raised. When rats turned out not to prefer the escapable shock condition, I worried that it was because the form of control was not "good enough." The avoidance experiment, which permitted the rats to prevent shock from occurring at all, was conceived to address this issue.

Regards, Bruce

Date: Thu, 8 Jun 1995 15:59:15 -0600
Subject: Re: Bruce's experiments

[From Bill Powers (950608.1400 MDT)]

Bruce Abbott (950608.1105 EST),(950608.1300 EST) --

I guess I was letting my imagination run away with me. I am reassured by your description of the "shocking experiences" of the rats, and also by the fact that you did so many studies ancillary to the main one, to determine critical

facts. Despite all my nit-picking, I am impressed by your talents as an experimenter, a _careful_ experimenter who takes little for granted.

I should mention my own major experience with shock, which was in the Navy while inside a cage housing a powerful transmitter that I was troubleshooting as part of a class. The POWER OFF sign was prominently posted, and I mistook the map for the territory. Squeezing into the cage, I laid my bare elbow on the wrong terminal of a 0.5 microfarad capacitor charged to 3000 volts. My next participation in the world was about 10 minutes later. This was probably the point at which control theory began to germinate in my mind: whatever firm beliefs I had had about human nature were blasted out of existence and I had to start over. Well, that's a theory, anyway.

RE: Catch-22

We're talking about two different experiments here, aren't we? One in which the rats could, in principle, reduce the shock rate to zero, and one in which no matter how quickly the rats responded, they would get at least 100-300 milliseconds of shock. From the control standpoint these are very different situations.

In the first case, there is some reason to think that the rats were maintaining the experienced shock rate very nearly at their reference level for it, and that if the remaining average shock rate had still been onerous to them, they could easily have responded more frequently and reduced it further. In other words, there is no question that the demands on their response capabilities were anywhere near the limits of performance.

In this case we have two possible explanations for failure to prefer the condition in which the rats have control. As you indicated to Bill Leach, "a finding of no-preference could indicate either that the rats were indifferent between the conditions or that they had not learned how to switch from one condition to the other." Since you demonstrated that the rats were able to switch conditions when given a warning signal, that leaves only the "no-preference" option as a reasonable alternative.

It is not clear from this, however, that the rats would not prefer the controlling condition over the no-controlling condition when there is any significant loss of control in one condition. In the controlling condition, the rats clearly respond at a sufficient rate to reduce the shock rate by a large factor, keeping it close to the presumable reference level. It is interesting that they continue to improve their control of the shock rate until it is only a few per hour: evidently, shock is highly disliked when it occurs too often, and "too often" is not very often.

However, the required rate of lever-pressing is very low, and the shock rate is very low. The rats would not, I venture, experience any significant loss of control because the error would be close to zero. In the other condition, the rats would experience the same low error without having to press the bar every 20 seconds or so; the error would still be close to zero. This would not be experienced as loss of control, either, would it? This would remove any basis for a preference even if rats did prefer control over loss of control.

I should pause here to remark that I would be very surprised to find that rats would have a preference for an abstract condition called "loss of control", or its converse, "being in control," apart from any lower-order consequences of losing control. This would give rats capacities to control at the principle level perhaps greater than we could reasonably expect. But I am interested in how we could use the data to support my prejudice, in a way that follows logically from the data. So I'm looking for loopholes that would have to be plugged to permit us to reach the conclusion that I fully expect we are justified in assuming. It's a good idea to plug loopholes, because occasionally they turn out to be drains down which our comfortable assumptions disappear.

In the second experiment involving shocks, we find a different situation. Here the rats always get shocked, and the only dimension in which they can control is the duration of the shock. Once again, the rats show no preference for the controllable condition over the uncontrollable one, and we must again ask why.

The loophole here is in what the rats would consider to be "in control" or "losing control." You point out that in the nominal controllable condition, the rats reduce the shock duration to about 1% of what it would be if they did nothing. The first crack in the argument appears when you ask what would have happened if you had made the automatic cutoff point 150 seconds rather than 15. You would then conclude that the rats were using _the very same behavior_ to reduce the shock duration to 0.1% of what it would have been without control. And if you had reduced the cutoff point to 1.5 seconds, and the rats continued to limit the duration to 0.15 second, now it would seem that the control has left a 10% error in shock duration -- still without any change in behavior.

What this would tell us is that we are using the wrong basis for judging the quality of control. In fact, you now report several side-experiments that support this conclusion:

> The 1.0 mA level is close to the minimum value that will produce rapid, reliable escape behavior. At 0.5 mA the escape latencies are longer as the rats are likely to be spending more of their time exploring the chamber, grooming, and so on, and thus tend to be some distance from the escape lever when the shock occurs. At 0.25 mA they may not acquire the escape response at all.

So evidently, at 0.5 milliamp the rats relax their efforts to respond quickly and take longer to press the lever. At 0.25 milliamp they may not press the lever at all. And at 1.0 milliamp they respond as quickly as they can. As you indicate, duration should also be taken into account, but presumably these were all shocks of the same duration.

So for shocks of whatever this duration was, we find that the fastest response comes for a current of 1.0 milliamp. The speed of response is then near the upper limit that the rats are capable of producing. If either the current or the duration were further increased, the rats could probably not respond in any significantly shorter time.

This tells us, or would tell us if we verified some details, that the rats responding in 100-300 msec are near the upper limit of the range of control. It also tells us that the intensity-time product is near the upper limit of the controllable range. From previous data, the intensity-time product representing zero error is slightly less than 0.25 milliamp times the duration. If the rats were able to keep the intensity-time product near that lower figure, they would be experiencing no loss of control. But if they were responding as quickly as they could, yet the intensity-time product was near the upper limit, they would be close to the condition of "no control" (or as you propose, "out of control").

The second ancillary experiment supports these conclusions:

> When the inescapable shocks were about 100 milliseconds or more longer than the _average_ duration of the escapable shocks, the rats preferred the escapable-shock condition. When the reverse was true they preferred the inescapable-shock condition. When the shock durations in the two conditions matched within 100 milliseconds, preference, if any, was below the sensitivity of the experiment to detect.

If the rats in the escapable-shock condition were at the upper limits of the control range (almost zero control), then the main difference between the two conditions was simply in the total experience of being shocked. The preference that was indicated was for the least amount of shocking. This, therefore, was still not an unequivocal test of the rat's preference for "being in control," or for "not being out of control."

So neither shock experiment could clearly distinguish between preferring fewer shocks from preferring to be in control. But either one could have been modified to improve this discrimination.

In the first experiment, you determined the reference level for experienced shock rate: the shock rate that occurred when the rat could easily prevent shocks altogether. You could vary the experienced shock rate in either of two ways: by shortening the interval between resettings of the timer, or by requiring more than one lever-press to postpone the next shock. The more rapidly the rat has to press the lever, the greater the number of shocks will be experienced under the controlling condition. So you could determine the upper limits of the control range. Finally, you could pick a condition comfortably in the lower range of shock frequency where you know the rat is definitely able to control and that the shock rate is definitely higher than the reference level. Using this same shock rate in the no-control condition, remove any preference for the lower shock rate as a consideration. THEN if the rats failed to prefer the controlled condition, you could say that they have no abstract preference for being in control for its own sake.

In the second experiment you could vary the shock current and determine the range of latencies. Then you would pick a shock current such that the rats were definitely not responding as quickly as possible, and where the experienced shocks were definitely above the reference level. Again, matching the shock rate under both conditions, you could then rule out preference for a lower shock rate, and if any preference did occur, reasonably attribute it to a preference for being in control (or maybe out of control, who can say?).

We see that to make the determination you were trying to make, some rather fine tuning of the experiment is necessary. There is no way you could have known that such a strategy was needed, without control theory. As it happened, the two experiments demonstrated two extremes of control: nearly perfect, and nearly absent. The question you were trying to ask required finding the middle of the range, which could only be done with an understanding of PCT.

> Psychophysical studies show that saturation is not a problem: perception of shock duration follows Weber's Law.

Yes, but in the second experiment the limit was imposed by the maximum possible speed of the action (or maybe the maximum possible error signal), not saturation of the perception.

> If you are correct, then there is no way to test for control of a logical variable when the output is a step function.

It is difficult to separate a higher-level controlled perception from control of lower-level perceptions on which it depends. You have to make sure that the lower systems would receive the same disturbances under all conditions, with the only difference being a disturbance of a logical relationship. This can be done, but it takes careful work with the lower-level systems first.

> I don't want you to get the impression that I miss your point.

Never thought so for a second.

Best, Bill P.

Date: Thu, 8 Jun 1995 18:24:05 -0500
Subject: <No subject given>

[From Bruce Abbott (950608.1750)]

Judge Ito wants the lawyers in the Simpson case to follow a strict format when they object: state the objection, the legal grounds, and perhaps a brief

supporting statement, without further argument. I thought I'd follow Judge Ito's requirement here:

>Rick Marken (950607.1545)] --

>>Bruce Abbott (950607.1355 EST)

>> My example was intended to convey the point that having control can be stressful if maintaining control is difficult.

> To the extent that control is not maintained there is error -- and stress IS error. But NOT having control is always AT LEAST as bad as having it, and usually FAR worse.

Objection, your honor. Beside the point and untrue. Assumes facts not in evidence.

>>Bruce Abbott (950607.1155 EST)

>> No real control system can keep the controlled perception exactly at reference at all times regardless of disturbance.

> The phrase "regardless of disturbance" is ambiguous. Do you mean "given disturbances that change over time" or "no matter what the disturbance happens to be"? If you mean the latter, then the statement is obviously not true since a real control system can keep p exactly equal to r as long as d is a constant.

Objection, your Honor. Argumentative. It's obvious in what sense the term "disturbance" was meant. Counsel is attempting to confuse the issue. Also, contrary to fact. In the basic control system there is no output to counter the disturbance -- constant or otherwise -- when the error is zero.

>Bruce Abbott (950607.1245 EST) --

>> Hmmmm. Rick, you don't have a CLUE as to what my motives are.

> You are correct. I don't know your motives because there is no such thing as a "motive". "Motive" is a dormitive principle that was used to explain behavior before people understood the nature of behavior as the control of perception. People don't have motives any more than flammable materials have phlogiston.

Objection Your Honor. Counsel knows full well what "motive" means: the value of some controlled variable is not at its reference state. Counsel is attempting to deflect the argument from the issue at hand.

> It's impossible to hide the perceptions one is controlling from someone who knows how to find out what they are (using the Test). That's what I meant when I said (950606.1900):

> When you know what to look for, controlled perceptions are really quite obvious. Of course, Bill Powers (950606.1440 MDT) picked up on it too.

Objection Your Honor. Not an argument. Counsel is only repeating his earlier statement. Also, contrary to HPCT. The person may be controlling another variable that appears correlated with the obvious one. Also, assumes facts not in evidence. Counsel has not conducted such Tests in any systematic, formal sense that would yield unambiguous answers.

> But if you're interested, I'd be happy to tell you what perceptions I think you are controlling.

Your Honor, my client would be most interested if counsel would be willing to share his insights with the court.

Regards, Bruce

Date: Thu, 8 Jun 1995 20:00:09 -0400
Subject: Re: Shocking Details

<[Bill Leach 950608.19:02]
>[From Bruce Abbott (950608.1105 EST]

Thanks, I have learned a few more new things.

-bill

Date: Thu, 8 Jun 1995 20:51:43 -0400
Subject: Filing an Amicus Brief

<[Bill Leach 950608.20:14]
>[From Bruce Abbott (950608.1750 EST)]

If it please the court...

Rick's comment that "... not having control is always AT LEAST as ..." is not necessarily true even though likely true in the vast majority of situations.

His initial sentence in the reference paragraph is the correct statement of the situation (though more need and will be stated about error later).

In the next exchange:

>>> No real control system can keep ...

>> The phrase "regardless of disturbance" is ambiguous ...

> Objection, your Honor. ...

The initial "No real control system can keep the controlled perception _exactly_ at reference at all times regardless of disturbance." IS true in the absolute sense. . and in that sense is completely irrelevant to the discussion at hand. BTW, in the absolute sense "No real CS" includes all engineered as well as biological systems.

Thus, counselor Ricks following assertion is completely correct from any practical view including detailed analytical.

Moreover, the objection suggests a serious misunderstanding of basic closed loop negative feedback control system operation on the part of Counsel. To wit: The statement that "In the basic ... there is no output to counter ... when the error is zero." is again clearly true but wholly irrelevant.

The error signal value will be at a non-zero value pretty much 100% of the time when control exists. However, the important consideration (both from a discussion/analysis point of view and, it appears from ample evidence, the control systems view) is that the amount of error will be very small as long as control exists and rise to near full possible value when "good control" is not obtained.

That is in the basic control loop, error signal value immediately jumps to maximum output as soon as the controller is unable to make perception match reference to within the deadband of the controller.

A common misconception about basic closed loop negative feedback control is that if the error signal possible output range is zero to 10 volts (for example) that one would see the error voltage "swinging" between these two extremes in normal operation with disturbances applied. The reality of the situation is that one would probably have to have a high sensitivity voltmeter with a full scale range of maybe 10 mV to see any change in error voltage at all.

> motive

The "friend of the court" agrees with Counsel here. The term "motive" is a badly used and abused term from conventional psychology but that fact does not mean that the term can not be used with a PCT understanding of what it would have to mean.

OTOH, Counsel's description of motive in his objection is a "nice sounding PCT like description" but probably not at all accurate.

The "friend of the court" suggests that first a "MOTIVE" is a perception. Secondly a "MOTIVE" is specifically a conscious perception. Thirdly these is associated with this "MOTIVE" the perception that it is a "reason" why certain goals are desired.

It is further suggested that "MOTIVE" includes conscious perception by others concerning why the subject might or might not be controlling a particular EV.

Since it is likely that in most situations and for most people, the subject is quite possibly incorrect in an understanding of its' own motives, the search for motive is an exercise in futility.

To make my point; The researcher applies a series of TESTs to determine and verify to the extent possible the potential CEVs of the subject. The researcher then "muddies up" an already doubtful situation into an impossible situation by postulating a "MOTIVE".

Further applications of the TEST could show consistency with the posited motive but can not "prove" this motive true.

The researcher then asks the subject what the MOTIVE is and compares the two. They most likely do not match (either substantially or in detail, matters not). The researcher is in error since the MOTIVE is the subject's perception (whether is correct or not).

>>> It's impossible to hide
>> When you know what to look for ...
> Objection ...

Have to agree with Counsel (especially with the added comment concerning HPCT).

-bill

Date: Fri, 9 Jun 1995 09:23:54 -0700
Subject: Motives

[From Rick Marken (950609.0920)]

Bruce Abbott (950608.1750 EST) --

> Objection Your Honor. Counsel knows full well what "motive" means: the value of some controlled variable is not at its reference state. Counsel is attempting to deflect the argument from the issue at hand.

"Motive", like "behavior", is a fuzzy concept in conventional psychology. However, the term "behavior" has a clear meaning in the context of PCT: "behavior" refers to the process of controlling perceptual variables; behavior IS the control of perception.

"Motive" can also be given a clear meaning in PCT but we have not spent much time doing it because there are far better ways to describe the aspects of control that are included in the meanings of "motive". To make sense, "motive" must refer to several aspects of control simultaneously: perceptual variable, reference state of that variable, error signal. So, when you say "you don't know my motive" you might be saying that I don't know some or all of these variables. I might, for example, know the perceptual variable you are controlling but not its reference level.

In my answer to you I tried to explain that I do have a pretty good idea what perception you are controlling and its reference level. I know what variable you are controlling, by the way, because I am controlling the same variable, which might be called the "relationship between PCT and conventional theories of behavior". But we are controlling this variable relative to different reference levels. That's why we are always getting into conflict; your efforts (verbal and modelling) to bring your perception of the relationship between PCT and conventional theories to its reference level (which I would call "PCT is the correct theory and the other theories are wrong") are a disturbance to my perception of the same variable because I am controlling that variable relative to a different reference (which I would call "PCT is the only theory of control;the other theories of behavior explain a different -- and non-existent -- phenomenon).

Best Rick

Date: Fri, 9 Jun 1995 12:18:53 -0500
Subject: Amicus Brief

[From Bruce Abbott (950609.1145 EST)]

>Bill Leach 950608.20:14 U.S. Eastern Time Zone --

Your Honor, we have examined the amicus brief filed by your "friend of the court." We find in his statements great insight and wisdom concerning all those cases wherein he agrees with our position. As to the rest, well, nobody's perfect. However, we believe he will come around to our side after we have had the opportunity to offer our response (and perhaps a small $$token$$ of our appreciation) in those areas where we appear to disagree.

> The initial "No real control system can keep the controlled perception _exactly_ at reference at all times regardless of disturbance." IS true in the absolute sense... and in that sense is completely irrelevant to the discussion at hand.

Your Honor, this is DIRECTLY relevant to the discussion at hand, in that we are arguing that stress may arise even when there is control. Control is never perfect, and error can lead to stress.

> Moreover, the objection suggests a serious misunderstanding of basic closed loop negative feedback control system operation on the part of Counsel. To wit: The statement that "In the basic ... there is no output to counter ... when the error is zero." is again clearly true but wholly irrelevant.

> The error signal value will be at a non-zero value pretty much 100% of the time when control exists. However, the important consideration (both from a discussion/analysis point of view and, it appears from ample evidence, the control systems view) is that the amount of error will be very small as long as control exists and rise to near full possible value

Your Honor, my client has logged time on DEMO1 and DEMO2 and understands full well how the basic control system operates. The Friend-of-Court assumes in his argument that the system in question is high gain and lacks significant lags. Neither of these assumptions is necessarily true of a given real control system, in which case the error may at times be significant even though reasonable control is maintained.

> The error signal value will be at a non-zero value pretty much 100% of the time when control exists. However, the important consideration (both from a discussion/analysis point of view and, it appears from ample evidence, the control systems view) is that the amount of error will be very small as long as control exists and rise to near full possible value when "good control" is not obtained.

Your Honor, this has not been our experience on compensatory tracking tasks. The error has certainly been less than the "near full possible value" and yet not "very small" either, large enough to arouse some degree of stress.

> That is in the basic control loop, error signal value immediately jumps to maximum output as soon as the controller is unable to make perception match reference to within the deadband of the controller.

Your Honor, a control system that "slams the needle" on the output as soon as there is detectable error by definition is a high-gain system and does not represent the general case.

> OTOH, Counsel's description of motive in his objection is a "nice sounding PCT like description" but probably not at all accurate.

> The "friend of the court" suggests that first a "MOTIVE" is a perception. Secondly a "MOTIVE" is specifically a conscious perception. Thirdly these is associated with this "MOTIVE" the perception that it is a "reason" why certain goals are desired.

> It is further suggested that "MOTIVE" includes conscious perception by others concerning why the subject might or might not be controlling a particular EV.

Objection, Your Honor. Motives may become objects of perception (one may be aware of one's motives) but often are not ("unconscious motivation"). Motives-as-perceptions are inferences as to what the motives are; they are not the motives themselves. We may be wrong about these inferences, both about our own motives and those of others, but that does not change the motive itself. To state the motive is to state what goal-state is being sought or defended.

Regards, Bruce

Date: Fri, 9 Jun 1995 21:26:25 -0400
Subject: Re: Amicus Brief

<[Bill Leach 950609.19:08 U.S. Eastern Time Zone]
>[From Bruce Abbott (950609.1145 EST)]

> We find in his statements great insight and wisdom concerning all those cases wherein he agrees with our position. As to the rest, well, nobody's perfect.

Interesting statement. Are you saying that IF I agree with you then my statements contain "great insight and wisdom" but IF I DO NOT so agree then they fail to contain "insight and wisdom"?

> Your Honor, this is DIRECTLY relevant to the discussion at hand, in that we are arguing that stress may arise even when there is control. Control is never perfect, and error can lead to stress.

There are a number of concepts here in which we need to "come to terms" over just to find out if we are even talking about the same things or not.

To start with, I absolutely agree that NO control system can control its' perception to exactly the reference value at all times. In an absolute and exacting sense, perfect control probably exists much much less than 1% of the time. This fact is irrelevant to the sort of discussion that we are having.

Control is "good" basically when the perception is maintained close enough to the reference that the control system is "satisfied". Not a terribly useful definition but it is a terribly important concept. It does not matter how effective the control appears to us observers, the only factor of import is whether the subject system considers control to be good or bad based upon its own criteria.

"Bad" control is the situation wherein the perception is NOT being maintained to within acceptable limits about the reference value.

Stress is the existence of unacceptable error. Much discussion has taken place on CSG-L concerning stress in the past. Some of the generally accepted conclusions:

> Stress itself probably is not a "real" phenomenon but rather a label that we apply to a property, that we "notice", that must exist in a self-organizing control hierarchy. (ie: There is probably no "Stress System").
>
> Further, the term Stress generally applies to a perception. That is when we "feel frustrated" or the like, we consider ourselves to be "under stress". The perception itself is undoubtedly related to the actual metabolic effects sensed that are themselves controlled perceptions.
>
> Increasing error generally means increasing stress value. Individual error signals probably have a threshold for onset of stress and grouped error likely do as well.
>
> Multiple small errors, each of which _might_ not individually be resulting in any stress probably combine to create a "general" stress level perception.
>
> There may be a "priority" associated with controlled perceptions (actually there undoubtedly is such). It appears that an inability to bring some perceptions under control at all is not "stressful" while even mildly poor control of others is very Stressful".
>
> The PIF for the reorganization system probably includes not only a priority sensitive scheme but probably also an integrator.
>
> Reorganization probably begins at some level of error above unacceptable.
>
> The rate at which reorganization proceeds is probably a function of the extent to which error(s) exceed their acceptable limits.
>
> If Stress is viewed as the activation of reorganization (ie: error amount is perceived to exceed threshold limit) then most "Stress" is not consciously perceived at all.

If a control system is controlling a perception to within acceptable limits then there is no resulting stress (with respect to that control system). Stress could result from other perceptions such a perceiving that one is "exhausted" or even that one "will" fail to control in the future. Even irrational perceptions can "cause" stress. For some people, the perception of stress itself causes stress (cardiac and ulcer patients might easily fall in this category).

So yes, control is never "perfect" and error _can_ lead to (a perception of) stress. Or excessive error IS stress depending upon just what you would like the term stress to really mean.

> Your Honor, my client has logged time on DEMO1 and DEMO2 and understands full well how the basic control system operates. The Friend-of-Court assumes in his argument that the system in question is high gain and lacks significant lags. Neither of these assumptions is necessarily true of a given real control system, in which case the error may at times be significant even though reasonable control is maintained.

Bruce, I am only going to say that in my over 30 years of experience with closed loop negative feedback control systems that HIGH OPEN LOOP GAIN is a basic fundamental requirement for successful control in all but very limited and very well designed environments. In my experience a control system displaying error values above a few percent of full scale is "barely hanging on" and will likely soon fail altogether. Momentary spikes in the error signal to even 100% of output are usually not of concern for most systems.

The internal signal values in the models that we create to run on digital hardware may in reality have very little resemblance to the equivalent signals in the "real thing".

> Your Honor, this has not been our experience on compensatory tracking tasks. The error has certainly been less than the "near full possible value" and yet not "very small" either, large enough to arouse some degree of stress.

Unless you have been making some measurements that no one else know how to even begin to approach making the connections, the "errors" that you are talking about are only incidentally related to the errors within the organism. In addition, knowledge of the magnitude of an error signal does not, at this point in time, give us a clue as to that signal's relationship to reorganization (or like system).

> Motive

Sorry, I don't buy the idea at all that there is something called a "motive" that actually exists as an entity within a living system other than as a conscious perception.

One of the very serious problems that exist in all such discussions today is that we so readily equate "described and labeled" phenomenon to actual functional requirements within the organism.

If the motive is the goal, then lets drop the term motive as it is far more ambiguous than the term goal.

"The Motives are the reasons why we do or want things" is a common definition for the term. It is also devoid of any objective meaning.

-bill

Date: Sat, 10 Jun 1995 19:40:13 -0500
Subject: Re: Amicus Brief; Stress

[From Bruce Abbott (950610.1935 EST)]

>Bill Leach 950609.19:08 U.S. Eastern Time Zone --
>>[From Bruce Abbott (950609.1145 EST)]

>> We find in his statements great insight and wisdom concerning all those cases wherein he agrees with our position. As to the rest, well, nobody's perfect.

> Interesting statement. Are you saying that IF I agree with you then my statements contain "great insight and wisdom" but IF I DO NOT so agree then they fail to contain "insight and wisdom"?

C'mon, Bill, that was intended to be funny. Guess I should have put the little "winky-face" after it to make that clear ;-> Where did everyone's sense of humor go on Friday, on vacation?

Thanks for the nice discussion on "stress," including generally accepted conclusions from past debates on the topic on CSG-L.

Regards, Bruce

Date: Sun, 11 Jun 1995 00:19:54 -0400
Subject: Re: Interesting Point

<[Bill Leach 950611.00:12 U.S. Eastern Time Zone]
>[From Bruce Abbott (950610.2020 EST)]

I am SURE that Bill was not making that point with respect to anything that you have said. Rather I suspect that it was a logical "follow-on" to where his discussion was leading. I know that I don't remember seeing you ever "refer to authority" (at least not in a position seeking way). All of your reference to "others" seem to have been to cite their work.

-bill

Date: Sun, 11 Jun 1995 00:20:05 -0400
Subject: Re: Amicus Brief; Stress

<[Bill Leach 950611.00:15 U.S. Eastern Time Zone]
>[From Bruce Abbott (950610.1935 EST)]

You are right but then you were not. I did presume that the statement was in jest (I don't view you as being an arrogant person)... but I did note that there was no "smiley" so I thought I would slam you back!

Thanks for the comment on stress. Seriously though, I probably should have written that up, saved it, and then reviewed and edited it for later posting.

That discussion was really an attempt on my part to point out some of the insidious difficulties that we face when attempting to discuss what goes on in the higher levels of HPCT. So often (most of the time?), the ideas that we label probably do not actually have a "real" existence within us that is even remotely similar to what we mean when we use the label.

-bill

Date: Sun, 11 Jun 1995 09:39:38 -0400
Subject: Re: Amicus Brief; Stress

<[Bill Leach 950611.09:34 U.S. Eastern Time Zone]
>>[Bruce Abbott (9506??.???? EST)]

Bruce;

Something else just occurred to me...

We were talking about "error" signal values in "normal" controllers and I talked about the behavior of the signal with respect to perception variation from reference conditions.

I think that we might have been actually talking about different signals. That is, I was talking about the "error" as it would be seen at the comparator output and I suspect that you may have been talking about the "error" signal viewed at the actuator input. Is this possibly correct?

The two signals would behave quite differently in most controllers. For one thing the actuator input seldom "slams the needle" in reasonable control situations even though the comparator output often does.

-bill